set of enemies, requiring numerous modifications of form, structure, and habits, the exact purpose of which we should have found it difficult to interpret. But here, where both competitors and enemies are at a minimum, we are able distinctly to see the few and simple modifications which have adapted the species to its changed mode of life. We have here, too, a case in which the isolation supposed to be essential in the production of new species has been effected solely by a change of habits within the same limited area, and it is evident that this mode of isolation would be equally effective in the case of a continental as of an insular species.

Lizards, snakes, and frogs are tolerably abundant, and the proportion of species peculiar to the islands is in the order in which they are here named; and this also indicates the increasing difficulty of transmission across an ocean barrier. Birds seem to be fairly abundant, parrots and pigeons forming the most conspicuous groups, while birds of paradise appear to be absent. Although insects decrease in number of species as we go eastward from New Guinea, yet two of the grandest of butterflies—*Ornithoptera Urvilleana* and *O. Victoria*—are found in the Solomon Islands, and were among the greatest treasures of Mr. Woodford's collections. The latter species was only known by a female specimen obtained by Macgillivray, the naturalist to the *Herald*, in 1854, till Mr. Woodford again found it in 1886, and discovered also the beautiful green and black male. Many fine Papilios are also found, among them a splendid blue and black species allied to the well-known *P. Ulysses* of the Moluccas. Here, as elsewhere in the tropics, some striking cases of mimicry occur, three species of Euplæa being so closely imitated by three species of Diadema, as to be undistinguishable on the wing; and each pair appeared to be confined to a separate island.

The following is an interesting observation on the habits of pigeons:—

> "The small islands on the reefs are much frequented by pigeons. They resort to them during the day, but mostly towards sunset, when, at some islands that I know of, the pigeons may be seen arriving by twos and threes, or in flocks of ten or a dozen each, to roost on the islands, until each tree is crowded with birds. The only reason that I can assign for this habit is, that on these small islands the pigeons are freer from the attacks of the large monitor lizards that abound on all the large islands. I do not consider this at all a satisfactory reason, but it is the only one I am able to suggest. Certain it is that this habit of the pigeons plays an important part in the distribution of seeds from island to island. On any of these small islands the large seeds of the Canarium nut tree may be found, after being disgorged by the pigeons, while young trees in different stages of growth may often be seen."

Mr. Woodford's explanation of the pigeons' roosting on the small islands appears to be a highly probable one, and quite in accordance with other facts relating to this tribe of birds. They are exceptionally abundant in tropical archipelagoes, and most so in those where, as in the Antilles, the Mascarene group, the Moluccas, and the Pacific islands, arboreal carnivorous mammals are very scarce or altogether wanting. An analogous fact to that noted by Mr. Woodford is, that although the beautiful Nicobar pigeon has an enormous range, from the Nicobar Islands to New Guinea, it is almost unknown in the larger islands, especially in the western half of its area where mammals abound, but is more especially confined to the smaller islets and reefs, where it is comparatively free from enemies.[1]

Although the natives of the Solomon Islands are well supplied with Bryant and May's wax vestas in metal boxes—the only kind of matches that can be kept in the damp atmosphere—they still make fire in the native way, by friction, on certain ceremonial occasions, or at other times when matches are not forthcoming; and their method of proceeding is well described by Mr. Woodford. It consists in rubbing a hard piece of wood in a groove formed on a soft dry piece—the method used in the Moluccas and Australia—and he tells us that, though a native will usually produce fire in less than a minute, he has himself rubbed till his elbows and shoulders have ached without ever producing more than smoke.

The following extract gives a fair idea of the author's style:—

> "It is amusing to see a mere child paddle alongside in a crazy trough of a canoe, only just capable of supporting its weight. The water splashes into the canoe at every stroke of the paddle, and at intervals the small child kicks it overboard with its foot—a novel kind of baler. Three or four mouldy-looking yams, ostentatiously displayed, are rolling about in the water at the bottom of the canoe. The unsuspecting stranger takes pity on the tender years, and apparent anxiety of the small native to trade, and gives him probably four times the proper price for his rusty yams. The child eagerly seizes the coveted stick of tobacco, and immediately stows it for safety through a hole in his ear, where at least it will be in no danger of getting wet. He next whisks aside a dirty-looking piece of matting that has apparently got accidentally jammed in one end of the canoe, and displays some more yams, of a slightly better quality than the last. For the sake of consistency you cannot well offer him less than you did before, and another stick of tobacco changes hands, and is transferred to the other ear. You think now that he must have finished, as there is no place in the canoe to hide anything else, but with a dexterous jerk that nearly upsets the canoe he produces a single yam that he has been sitting upon. How it managed to escape notice before is a puzzle. For this he demands a pipe, but is not satisfied with the first or second that is shown him. No; he must have a *piala tinoni* or have his yam back. The *piala tinoni* is a pipe with a man's face upon the bowl. But again the young trader is particular, it must also have a knob at the bottom or he will have none of it."

The book is well got up, well illustrated, and very pleasantly written. It is full of information as regards the natives, the scenery, and the natural history of these little-known but very interesting islands, and can therefore be confidently recommended to all who care for books of travel in little-known countries.

A. R. W.

OUR BOOK SHELF.

Recherches sur les Tremblements de Terre. By Jules Girard. (Paris: Ernest Leroux, 1890.)

THE scientific study of earthquake phenomena has of late years made great progress, and we are glad to welcome a book which brings together the new matter

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[1] See "The Malay Archipelago," p. 350.

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which has hitherto been published only in various Journals and Transactions of Societies. The book commences with a chapter on ancient traditions, giving a chronological table of the more important shocks which have occurred since 79 A.D. The second chapter briefly discusses the connection between earthquakes and volcanoes, a subject of which we have apparently a good deal still to learn. Then follow descriptions and illustrations of various seismometers and seismographs, including the latest forms devised by Profs. Gray and Milne. In this chapter there are given several interesting comparisons of earthquake curves automatically recorded by the instruments, and curves artificially produced by the application of forces of known direction and magnitude. The propagation of shocks through land and water, and their destructive effects, are also considered, the latter being illustrated by sketches of some of the more remarkable fractures and displacements which have been observed. The last chapter summarises the suggestions which have been made as to possible connections between earthquakes and astronomical and meteorological phenomena. In conclusion, M. Girard points out the necessity for continued systematic observations, and enumerates the chief points on which further information is required.

To those who know little or nothing of the subject, M. Girard's little book will form an admirable introduction; and to the initiated it will be a handy book of reference to its latest developments.

La Photographie à la Lumière du Magnésium. By Dr. J. M. Eder. Translated by Henry Gauthier-Villars. (Paris: Gauthier-Villars and Son, 1890.)

THIS is a translation of a very interesting little German work on the employment of magnesium light for the purposes of photography, and will form a useful addition to our photographic literature. The author first gives a brief account of the earlier stages of the subject, taking us back to the time when Bunsen and Roscoe, in the year 1859, indicated the considerable advantages the light of magnesium presented for photo-chemical studies and lighting. He then shows how Crookes afterwards employed the light for photographic purposes.

Amongst the very first attempts of artificial lighting, the wire of magnesium was used. It was burnt in a specially-made lamp, and the light thus produced answered fairly well for interiors, but was useless for portrait work, being too harsh. The next advance was the employment of a mixture consisting of the powder of magnesium, chlorate of potassium, and a sulphide of antimony; the light was produced by igniting the mixture, which flared up instantaneously. The chief drawback to this method was the great precaution that had to be taken during the mixing, as the slightest blow caused an explosion. Saltpetre in place of potassium was sometimes used so as to lessen the chances of explosion.

The methods described in chapters v. and vi. were those which gave the best results. They consisted in blowing powdered magnesium through a tube and allowing this powder to come out at the other extremity into a gas or candle flame; the light thus produced was extremely actinic, and did not present any danger. The lamps of Schirm and Loehr, illustrations of which are given in these chapters, were on this principle, and gave great satisfaction for portraiture, being worked by means of a pneumatic india-rubber ball. Chapter vii. treats of the combustion of magnesium in oxygen, and in it is described Piffard's apparatus for the production of this light, which was found to be enormously increased by the presence of the oxygen. The remaining chapters deal with methods of taking groups by this artificial light; and there is a very interesting illustration of the pupil of the human eye, photographed in a dark room by means of the flash light, the exposure of which was so short that the pupil had no time to contract. The book concludes with some hints on the precaution necessary to insure successful development of the negatives taken by these processes, and with a short appendix by M. Alexandre.

LETTERS TO THE EDITOR.

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Panmixia.

BUT for his statement that I "cannot be sincere," I should not have deemed it necessary again to answer Prof. Lankester; anyone who is read in the literature of Darwinism must already have perceived that a further reply on my part is needless. An accusation of insincerity, however, ought not to pass unnoticed; and therefore I will ask your more general readers to observe the ground on which it has been made.

In my answer to his original criticism I endeavoured to show that Prof. Lankester "fails to distinguish between the cessation and the reversal of selection," or, more particularly, between panmixia and the economy of growth; and this is the point with regard to which insincerity is charged. Yet this is just the point—and the only point—in dispute. I have always represented that the cessation of selection is *per se* a cause of degeneration, whether or not it be associated with the economy of growth. Prof. Lankester, on the other hand, represented that the cessation of selection is not *per se* a cause of degeneration; but merely a "state," which is precedent to, and contemporaneous with, the economy of growth—the latter being the cause, while the former is but a condition to the occurrence of this cause. Such, at any rate, appeared to me the only meaning that could be gathered from his paragraph at the top of p. 488; and it is now over and over again repeated in his last letter. For instance:—"Cessation of selection *must be supplemented by economy of growth* in order to produce the results attributed to 'panmixia.' And inasmuch as economy of growth as a *cause* of degeneration involves the *condition* of cessation of selection, Mr. Darwin *in recognizing the one recognized the other*. . . . It is true that Mr. Darwin did not recognize that such unrestricted variation must lead to a diminution in size of the varying part *without the operation of the principle of 'economy of growth.'* This was no strange oversight: *he would have been in error had he done so*. . . . The term ['panmixia'], like its correlative 'cessation of selection,' *does not indicate a principle, but a natural condition:* it does not involve the inference that a dwindling in the size of the organ must result from inter-breeding; but *simply points to a precedent condition*" (p. 559: italics mine).[1]

Where, then, is the insincerity in saying that Prof. Lankester does not perceive the distinction between the cessation of selection and the economy of growth as two totally different causal "principles"? Or what remains for me but to repeat, with all sincerity, "he confounds the 'idea' of panmixia with that of the economy of growth," and "fails to perceive the 'essence of the idea' in the all-important distinction between selection as withdrawn and selection as reversed"?

It is true that at the close of his last letter Prof. Lankester admits, "when we consider shape and structure, and not merely size, it is clear that panmixia without economy of growth would lead to a complete loss of that complex adjustment of parts which many organs exhibit, and consequently to degeneration without loss of bulk." But how was it possible to surmise from his first letter that he had in his mind such reservations as to "shape" and "structure?" Or, indeed, how is it possible to reconcile such reservations with the passages above quoted from his last letter, to the effect that the cessation of selection is "not a principle at all," but merely "a condition which alone cannot produce any important result"? Are we to conclude that in Prof. Lankester's opinion neither "a complete loss of complex

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[1] I may remark that the term "cessation of selection" is not the "correlative," but the *synonym* of the term "panmixia." And I may further remark that the term "reversal of selection" is not, as Prof. Lankester supposes, the synonym of the term "economy of growth." Economy of growth, where useless structures are concerned, may determine a reversal of selection; but the reversal of selection may also be determined by many other causes and conditions, which are equally potent—or even very much more potent—in this respect.

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